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What follows from this is a generalization of some scope: the genesis of reason is to be sought in imagination; there may even be an eventual conformity of the ideals of these two faculties, an eventual identity of the true and the attractive.

Of course I would not wish to say that the mere fact of the genetic unity *necessarily* means the teleological unity of the mind's activities. But there are grounds for asserting that it *may* mean this. Human nature, with all its diversities, is still an organic whole; the condition of its being is the harmonic action of its parts. The mere fact of the enormously costly and cumbersome evolution of reason is guaranty of its biological value—of nature's sincerity in creating it for the whole organism. We all know the like value of the more ancient conative side of the mind. Have we not, then, warrant to suppose that the sapience of reason is vouchsafed for some better realization of those ideal satisfactions which the imagination so stoutly labors to supply? Certainly the fact that the chief category of our exact knowledge appears to be a direct specialization of the chief category of our imaginative, motivating, knowledge would seem to argue consistency in nature's intent.[4]

Such an interpretation might very possibly smooth over some philosophical antagonisms.

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REVIEWS AND ABSTRACTS OF LITERATURE

Yale Psychological Studies,[1] New Series, Vol. I., No. 1. *Monograph Supplement, Psychological Review*, Vol. VII., No. 1, March, 1905. New York: The Macmillan Company. Pp. 226.

The various papers on the different illusions report a very interesting and successful attempt to photograph the movements of the eye in looking at the various lines. As a piece of psychological mechanism, the work done shows the greatest amount of thoroughness and care. The initial difficulty met with was the lack of any natural means of getting a fixed and identifiable spot on the eyeball which could be used for the pictures taken. The bright spot of the eye could not be used, since this is a variable quantity, changing with each rotation of the eyeball in its socket. To obviate this difficulty, a white spot was painted on the cornea by means of flaked Chinese white, the vehicle being paraffine. The head of the subject was supported by a device in the back, and by a 'bit' in the front, in the usual place, between the teeth. Iron spectacles, with shining steel beads on the lower rim for purposes of reference, were also worn by the subject. The photographs taken (at about the rate of eight per second) were then projected by lantern on a sheet of white. Since the iron spectacles with beads remained in the same place, the positions of the white spot in its wanderings were accurately marked and numbered as picture after picture was thrown on the screen.

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[4] If illustration be needed, the history of music is in point. Here the distinction of the quantitative, or mathematical, and the qualitative, or artistic, ideal of the study of sound, is as ancient as Aristoxenus and the Pythagoreans. As M. Le Dantec notes (*op. cit.*, p. 34), the two interests have developed wholly diverse languages, that of acoustics, that of musical art. "Ces deux langages sont exactement equivalents; l'un d'eux est plus complet que l'autre; par example n'importe quel bruit sera justiciable de l'analyse acoustique et pourra être reproduit par un phonographe, tandis qu'il sera impossible de le définir en langage musical. Le langage musical est limité a un certain nombre de sons choisis par les hommes d'après le plaisir qu'ils trouvaient à les entendre, et ayant par conséquent un rapport indéniable avec la nature humaine; notre gamme fait hurler les chiens." In other words, the exact, comprehensive language is that of acoustics, but the significant utterance is that of music proper. The ends of the latter are those for which both interests work; improved musical instruments follow the researches of Helmholtz and Lipps; broadened esthetic appreciation ensues upon the theories and achievements of Wagner.

[1] The contributions in the supplement are as follows: 'Introduction to a Series of Studies of Eye Movements by Means of Kinetoscopic Photographs,'

In the photographs of the eye movements it is shown that in fixation of a point, the eye does not really fixate a point, but rather a small area. It goes round and about the point, the image of the point, therefore, taking position on various parts of the fovea. Another interesting fact brought out is that the two eyes are only approximately coordinate in fixation, or in passing over a line from one end to the other. In such passage, however, the eye is able accurately to follow a line from some given point to another. It might have been of interest to find what relation the area of fixation bears to the area of the fovea. It might be possible with the data in hand to determine whether the retinal area relative to fixation is the fovea, and whether fixation areas can be calculated for given distances of fixation.

Three papers in the monograph apply the method above described to the determination of eye movements in looking at the three given illusions. In the Müller-Lyer illusion, "the subject was directed to begin either at one of the extremes or at the middle where the obliques meet the horizontals, and to look across the figure in one direction and then back, and so on, fixating each time the three points of intersection of horizontals and obliques. This movement was to be made slowly enough to allow the subject to see the illusion clearly in the course of each inspection" (p. 55). The photographs show that the eye makes restricted movements in the underestimated part of the illusion, from which Judd concludes

by Charles H. Judd, Cloyd N. McAllister and W. M. Steele, pp. 1–16. 'The Fixation of Points in the Visual Field,' by Cloyd N. McAllister, pp. 17–53. 'The Müller-Lyer Illusion,' by Charles H. Judd, pp. 55–81. 'The Poggendorff Illusion,' by E. H. Cameron and W. H. Steele, pp. 83–111. 'The Zöllner Illusion,' by Charles H. Judd and Henry C. Courten, pp. 112–139. 'Analysis of Reaction Movements,' by Charles H. Judd, Cloyd N. McAllister and W. M. Steele, pp. 141–184. 'Practice Without Knowledge of Results,' by Charles H. Judd, pp. 185–198. 'Movement and Consciousness,' by Charles H. Judd, pp. 199–226.

that sensations of eye movements are not satisfactory as explanation of the illusion. The conclusion, however, can hardly be justified. As data for eye movements over an illusion, such movements being made under direction, the results are accurate and trustworthy. But as a study of illusion, they are hardly available. The restricted eye movements might be due to the inturned ends of the intersecting lines, to caution necessitated by small space, *such space being already seen,* and only partly fixated. Might it not be that (where the lines are not too long) the space is already seen, the smaller space hampering movement and so causing interruption? The underestimation may be the cause of the interruption instead of the reverse. *The illusion is seen before any extensive eye wandering has taken place,* and any eye movements occurring after the illusion is seen would be the results and not the causes.

In the experiments on the Poggendorff illusion, two of the five observers were asked simply to look back and forth over the presented figure. The results show most accurately the positions of the eye in following the line. It is shown that "the first portion of the movement conforms generally to the length and direction of that portion of the oblique line of the illusion over which it passes" (p. 102). The movement then shows a period of retardation and a significant change of direction. The movement then goes on uninterruptedly across the intervening space. There is then another series of pauses, after which the final stage is in the general direction of the oblique line. It seems highly probable, from the nature of the illusion, that these first movements determine the illusion, after which the movements may or may not be the cause. Since this illusion is one of direction, it seems to me that in this case, if the figure be not too small, some movement is necessary before the figure is seen in its entirety, and the nature of the movement brings about the illusion. The first (Müller-Lyer) and the last (Zöllner) illusions are rather ones of space.

In the Zöllner illusion tests, the observer was asked to look up and down all the long lines in any order he pleased, and asked to notice as carefully as possible the illusion. These movements are carefully plotted. In this illusion, as in the first, the illusion seems to exist before much movement has taken place. The movement, it seems to me, comes after the illusion exists, and seems to be an illusion partly of space, rather than of direction as is the Poggendorff. The plotting, it seems to me, shows not the movement giving rise to an illusion, but rather the curious investigation of an illusion already present. Exactly how much movement has already taken place at the moment the illusion stands out, would have to be determined, and all remaining movement would then be simply a result. A curious fact brought out is that the movement along the middle line is in a direction opposite to that in which the line appears to be deflected. Since, as stated in the paper, the habit of reading from left to right might be given as an explanation of the deflection, it would be interesting to see how the illusion influences those who read from right to left, or from the bottom up, and also how the deflection is seen by those who are unable to read, whether children or men.

As studies of eye movement under certain conditions, these experiments are worthy of the closest study, and should be carefully gone over by any one working in that line. But *as studies in illusion*, they are hardly available. Whether or not the angle explanation is valid, can not be determined by tests *with another means.* Even if the eye movements told us that the illusion depended on them, this would hardly shut out the angles as possible aiding causes. Where, as is highly probable, we have a mixture of effects, the methods of residues should be followed, and to determine the influence of the angles, the method of concomitant variations should be pursued *with the angles.* The authors, it appears to me, seem too anxious to be satisfied with simple explanations. It is possible by artificial means to produce eye movements similar to those made in looking at the illusion, without any angles being present. If, then, the illusion persisted, we might infer something from eye movement alone. The chief value of the experiments is their close connection with the sensori-motor theory of Dewey, and Judd himself recognizes this in his final paper on the relation of movement to consciousness.

In all these illusions, in addition to the photographic work done, a series of tests was carried on to find the effects of practice, which results are carefully plotted in curves. Mostly all the curves show a descent more or less steep. The general effect on the illusion is to bring about its disappearance.

The analysis of reaction movements deals chiefly with the antagonistic reaction in response to a stimulus after preparation. The method of reaction was of the 'press the button' kind. By a system of levers and springs the adjustments, vibrations, attitudes of the subject before and after the signal and stimulus were accurately recorded by means of a kymograph. "The records amply justify the statement that there is no such thing as a single reaction movement. Between the warning signal and the stimulus there is always a complex process of adjustment, and when the stimulus arrives, it finds the hand and the nervous system of the subject in some state of preparation which determines at once the way in which the reaction movement will be executed. There is no subject whose reactions are uniform or simple. Some are, indeed, more nearly uniform than others, but nearly every individual record shows complexity" (p. 163). The different types of reaction may be classified as the antagonistic, the wavy and the balanced. In the antagonistic reaction there is a general tendency on the part of the subject to prepare for the reaction by gradually or suddently moving in the opposite direction. The wavy reaction seems to be due to a normal rhythmic tendency, the reaction being shorter when the tendency at its highest point coincides with the stimulus. The wavy line marked on the paper may be attributed to a general nervous rhythm. From this point of view, the level line would signify expressive activity, since it shows a rigidity in the subject due to an effort to overcome the natural rhythmic tendency. These various changes in movement between the signal and the stimulus, it seems, were made without any consciousness of them by their author.

A rather interesting series of experiments are recorded by Judd showing the effects of habit without any knowledge of the fact that the habit was operating. A number of lines were drawn, some running up from left to right (+), and some down (—). A table was divided in two parts. On the left side, and in the field of vision were nine papers, one line on each. The right part of the table was out of the field of vision, being hidden by means of a screen. When a given line appeared, the subject was required, blindly, to put a dot at the place which he considered to be a continuation of the line, such dot being placed on the right-hand side. After a series of tests with the nine lines, it was found that no habit was formed, Judd concluding therefrom that this was due to the fact that there was no knowledge of results, and thus no motive for improvement. The corrective modifications due to knowledge of results were lacking. There is, however, another factor which seems to have been overlooked. The order of the lines given was, —60°, —15°, —45°, +15°, +60°, ± 0°, +30°, —30°, +45°. We have here a succession of conflicts which would operate strongly against the formation of any habit. With a single line a more rigid test could be made of this hypothesis.

A number of tests were then made with lines +60° and —45°. The subject was allowed at intervals to pull aside the screen and to see at the same time the white paper on which he was to indicate the projection, and the paper having on it the line +60°, without moving his hand while looking. The new conditions operated by causing some variations in the results at first, but this error finally disappeared, and a habit for +60° was formed. On the other hand, these conditions, favorable for +60°, operated against accuracy in the case of —45°. After a new series of tests with the nine lines as at first, the +60° habit was evident. A series of further tests was then made to see to what extent the +60° habit was effective. In a last series of experiments the subject was allowed to place his pencil in full view of both line and blank before doing it blind. This and other preceding tests were finally effective in disintegrating the +60° habit. As the author states, these tests show conclusively that where results are unknown, a habit operates against positive reaction a different way, and is only modified where its misapplication is seen and understood. This, it seems to me, is a form of variation due to subjective peculiarities, and which is allowed for in the 'personal equation' in different fields.

In the final paper of the monograph, Judd tries to bind the preceding papers together by an examination of the relation of movement to consciousness. He criticizes Münsterberg's '*Aktionstheorie*' as not fully covering all cases of motor adjustment. He cites against the '*Aktionstheorie*' those cases of vivid consciousness in which action seems impossible just because no motor channels appear to be open, and inclines rather to McDougall's view that the vividness is due to the necessity of *opening* a motor channel. I do not think that Judd does full justice to Münsterberg's position. Action does not mean in its fullest intent, throwing a fit, for example, or standing on one's head. A motor tendency, an attitude or an alternation of attitudes, is sufficient, consciousness of such attitudes giving us the vivid aspects of the present moment. Nor can the *opening* of a motor channel satisfy conditions, for this would carry us back to the cerebral psychology of the associationists, or to the misunderstood '*Innervationstheorie*' which has been discarded even by Wundt. The beginning of motor response to sensory stimulation would be nearer the truth. As Judd points out, Dewey has most adequately expressed the actual state of affairs by his 'sensori-motor' theory. "The sensation or conscious stimulus is not a thing or existence by itself; it is that phase of a coordination requiring attention because by reason of the conflict within the coordination, it is uncertain how to complete it." (Dewey, *Psy. Rev.*, 1896, p. 368, quoted by Judd, p. 202.) This sensori-motor theory is then examined in the light of the preceding experiments of the monograph and offered as a satisfactory explanation of them.

As a general suggestion in regard to the diagrams, tables and plates in this monograph, and also in other works, I offer the following: Could not each table, etc., have below a clearly printed set of explanations, etc., to facilitate reference? The constant reference back and forth is most confusing, and, as a matter of fact, most readers pass merrily by these tables, which are most laboriously gotten up for the purpose of elucidation. A proper set of explanations, directions, etc., clearly printed under each table, if necessary the whole thing occupying a separate page, would help the reader in getting the most out of them.

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An Introduction to the Theory of Mental and Social Measurements. EDWARD L. THORNDIKE, professor of educational psychology in Columbia University. New York, The Science Press. 1904. 8vo. Pp. xii + 212.

The few unimportant errors and omissions in this admirable book have been pointed out by various reviewers. I shall use the limited space that can be given to it here to speak of three particular excellencies, which make it the best introduction to statistical theory yet published, and a text-book that ought to mark an important departure in college training.

To begin with the simplest matters first, Professor Thorndike's more strictly practical chapters, on the arithmetic and algebra of the actual operations proper to statistical work, are marked not only by the simplicity and clearness of statement that are indispensable in such writing, but also by the ingenuity of a mind that has attained extraordinary cleverness in dealing with concrete cases. Students not altogether unfit for statistical work have had their difficulties with Bowley and Davenport. No student who can 'scrape through' the mathematics of his freshman year can fail to master Professor Thorndike's chapters on 'The Arithmetic of Calculating Central Tendencies and Variabilities,' 'The Transmutation of Measures by Relative Position into Terms of Units of Amount,' 'The Measurement of Differences and Changes,' and 'The Measurement of Relationships,' if he applies his mind to the task. These chapters alone would make Professor Thorndike's work more useful as a practical handbook in statistics than any other.